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THE UNIVERSITY AND RESEARCH[1]

THE main sources of research in America have been, and must continue to be, the universities. We have a few first class special research institutes; and we have a good many laboratories of industrial research and development. There are more of these than is popularly known; five hundred, perhaps, counting many small ones. But their work is directed more towards the attack of specific problems of development in the special industries which support them, and less towards the fundamental science that underlies these industries. In some of the larger of these industrial research laboratories, however, able investigators are at work and fundamental research of a high quality is carried on. But in all of the few special institutes and the many industrial laboratories taken together the research output is much less than that which comes from the universities. In addition, of one thing very important to the maintenance of research in the country these special institutes and industrial laboratories do almost nothing at all. That is the development and training of new research workers. This is done almost exclusively in the universities and colleges. Anything, therefore, which lessens the interest and activities of the universities in research, and hence reduces their actual output of research and research workers, is a menace to our national strength and well-being. For this strength and well-being depend, in a very large measure, on scientific research and discovery.

The conspicuous role played by science in the war from its very beginning, and the pressing necessity for solving serious war problems involving scientific investigation, brought very

[1] A paper read at the educational conference, May 13, 1921, held at the University of Minnesota, in connection with the inauguration of President L. H. Coffman.

vividly to the attention of the world the advantage to a nation of large scientific resources, both in equipment and personnel, and of being able to mobilize quickly and effectively these resources to aid in meeting the great emergencies created by war conditions.

At the beginning of the war Germany revealed herself far better prepared than any other country to take swift advantage of her scientific resources. In my interesting conversations in 1915 and 1916 with officers of the German General Staff at their great headquarters in occupied France, where I had to reside for some months as chief representative for occupied France of Mr. Hoover's relief organization, I was much impressed by the reliance placed by these officers on the help they were receiving and could always expect to receive from Germany's scientific men. When things were going badly on the West Front they would say: "Well, just wait; our scientific men will give us something new. They are all organized; they are all working; they will have something new soon to make your eyes stick out."

It is familiar history now that Germany's science, brought to the aid of her armies and navy, did repeatedly make our eyes stick out, and it was necessary before the war could be won to meet German's science with English and French and Italian and American science. We and the Allies had to organize science, too, and, with a haste made desperate by necessity, it was done.

Out of the revelations and experiences of the war came a great recognition and stimulus to the development of science which has resulted in the setting up by America, and several other nations, of new scientific organizations for the encouragement and support of scientific research and its applications by methods giving special attention to cooperative and coordinated work. Such methods involve an attempt to introduce a certain degree of organization into scientific investigation beyond that heretofore usually attempted.

The phrase, "organization of science," produces an unfavorable reaction from some scientific—and some non-scientific—men. It seems to suggest to them attempts to control scientific genius, to dominate scientific endeavor. They say that genius can not be organized, that scientific research must, like creative art, be left absolutely free from restraint. They ask if Galileo and Darwin and Einstein could have done greater things, or even the great things they have done, if they had been "organized." The implied answer is an emphatic No. And it may be accepted as the correct answer. But the question implies something that is not necessarily implied in the phrase "organization of science."

I know of no one in the National Research Council, nor do I believe there is any one in the new Department of Scientific and Industrial Research in England, or in the Bureau des Recherches in Paris, or in the National Research Council of Japan, who dreams of suggesting the advisability of organizing, or in any way interfering with, the individualistic work of scientific genius. What is suggested as advisable, because it was proved to be possible and highly effective in our war-time efforts, is to arrange for planned concerted attack on large scientific problems, especially such problems as require numerous cooperating workers and laboratories representing, often, not alone one special field nor even one major field or realm of science, but several such fields, as chemistry and physics, or chemistry and biology, or chemistry and physics and biology, or biology, geology and engineering, any one of which, and other, combinations, may be involved in the solution of large scientific problems affecting the national strength and welfare.

But even the isolated, individual workers may profit by the attention and encouragement and material support that may be given them by a coherent body of scientific men bringing to bear their collective influence to ameliorate the too often difficult conditions under which the isolated scientific investigator has to work, and to develop a wider appreciation, and hence public recognition and support, of scientific research.

What is the significance to the universities of this increased attention and new impetus

to research? And what is its significance to education and its methods? I suggest that this matter is so important that it requires a new and particular examination of the university and general educational situation as regards research and training for research.

The National Research Council has tried to become acquainted in some more exact degree than could be achieved by a perusal of college catalogues, or even an extended question and answer correspondence, with the present research situation in the colleges and universities of the country by means of a protracted series of personal visits by representatives of the council to some of these institutions. Up to the present one hundred and forty colleges and universities have been thus visited. This number includes enough institutions and institutions of enough variety to give us a fairly clear idea of the status of research and training for research in the colleges and universities of the land. Some day we may be inclined to publish a report or discussion of this situation as based on the information derived from these friendly visits.

But, for the moment, we may assume that we are all sufficiently informed in general of the state of our institutions of higher learning and the state of American higher education to warrant me in expressing certain opinions about the matter of research in the universities which your own knowledge will enable you to reject or confirm.

In the first place university research confronts a serious difficulty inherent in the very make-up and method of the peculiar American institution we call university. This institution *is* a university because it does university work. It isn't a university because it does much work that is not university work. To house under the same roofs, mix in the same laboratories, lecture- and class-rooms, and have sitting at the feet of the same instructors both preparatory and university students, is to produce an educational situation unique and very difficult—I should call it impossible—of successful carrying on. It is carried on, but not too successfully.

In the second place to give most of the attention, energy and money available to this curious institution to the preparatory students in it, because there are more of them than of the advanced students, is to place in secondary position the real university interest and members of this institution.

In the third place to give more attention and effort, as we really do, to the less capable, the uninterested, the non-attaining students than to the more capable, the interested and the attaining students, both in preparatory and university groups, is a menace to the highest usefulness of the institution if it is to exercise its true university function, which is the development of thinkers and leaders for the country. We may all be equal in our right to receive a common measure of service from the state but we are not all equal in our capacity to give service. The state, which is simply all of us, needs the benefit of the best use of our best brains, and to get it we must see that these best brains have the best of training.

In the fourth place to encourage the non-intellectual activities of the institution, such as its mere expansion in size, its display, its prowess in athletics, at the expense of its truly intellectual activities and achievements, is not grateful to the eyes of believers in the great need and importance to the nation of a university system of highest standard.

All these conditions, characteristic of the present American university, as I believe all you who know our universities intimately will admit, are serious difficulties in the way of the successful prosecution of research and training for research in these institutions. And this is true even when we construe research not in the narrow sense to which a growing technical and reverential use of the word tends to limit it, but in the most generous way in which it is entitled to be used, namely, simply as a going on in the quest for knowledge from that which is now known to that which is now unknown.

These conditions to which I have just referred are not only difficulties in the way of the development and achievement of research, and specific training for research, as such, but

they are difficulties in the way of the highest development of the whole intellectual atmosphere and achievement of America. The "Failure of the College" about which Professor Chapman, of Yale, writes so vigorously in a recent number of *School and Society,* is not simply the idle or sensational phrase of a sick pessimist but it is a phrase that well expresses the thoughts of almost all of us and that makes almost all of us feel sick as we face the facts. Yet we all go on; the colleges and universities all go on the usual way, as if the whole situation were out of our hands and on the lap of the gods for outcome. We act as if we were helpless; but that we should really admit that we are helpless is incredible. It is not American. It is not what we did when we faced the enormous problems of war. Do we have to have war to be capable? I am every day growing more impressed with the simplicity of war. War, which is supposed to bring complexity, brought us to simplicity and directness of thought and action; while peace, which should bring simplicity, has brought us to a perfect maze of complexity. No thing was too bold for us to attempt and achieve when we were at war. No thing seems capable of direct attack and solution now that we are at peace. But that is the sick pessimist again. And sick pessimism must not rule us. I am sure it will not. It is incredible that in this all-important matter of getting our higher education straightened out right we shall go on indefinitely acting as if we were helpless. Let the college or university that wishes to do the greatest thing just now to be done for higher education and true learning in America step forward and boldly do the unusual thing. Let it devote the most of its energies to the most important part of its work. It will soon not be alone in its doing. It will become a prophet with honor in its own land.

The National Research Council has recently interested itself in an inquiry as to what is being done to discover and encourage the students of superior capacity and attainment in the colleges and universities. One of its representatives has visited, since the first of February, about fifty institutions on this quest and for the purpose of friendly suggestion. He finds a lively appreciation of the importance of the matter, but a rather faint heart about doing anything about it.

In an interesting report recently made by this visiting representative, Professor George W. Stewart, of the University of Iowa, to the Council's Division of Educational Relations, certain impressions gained from his visits were expressed as follows:

> Although each ambitious teacher is anxious to develop leadership, yet, on the whole, when judged by distribution of time his emphasis in fact is laid on helping the mediocre in ability and the deficient in attainment. At first consideration, a teacher is inclined to deny the accuracy of this statement, but the visitor has found that after a brief discussion there results a fairly unanimous assent to its truth. The individual encouragement of the student of ability is one of the delights of the professor, but his class-room efforts are of necessity gauged for the average and the presence of the passing mark attracts his attention automatically and constantly to those of low standing. In other words, the routine demands a constant interest in others than the most able.
>
> None of the colleges visited is approximating its maximum service in the encouragement of superior attainment and in the detection and development of superior ability. The methods used by any institution have been adopted fortuitously rather than as a consequence of a definite sustained study of the problem. This is to be expected since the problem is really every one's business and concerted study and action is only given spasmodically when some one member has a single proposal that can be presented to the faculty in a form for action. This is not to be regarded as indicating a criticism of the professor but rather a weakness in our system.
>
> Numerous colleges are utilizing mental tests in one way and another, but, because of the time and hence money required, such an activity is not as widespread as it should be. Obviously such tests would be helpful in detecting superior ability. The inactivity of other colleges in this matter can be accounted for by the fact that no individual has the time to devote to it and the administration or the faculty is not adequately informed concerning mental tests.

This matter of the utilization of mental tests in helping to discover the students of

superior capacity and hence possibility of superior attainment, involves a wider recognition than now exists of the positive use which modern psychologists are more and more making, in their development of the applications of psychological science, of the fact that not only are there such marked differences in native intellectual capacity or ability among persons as to permit the setting up, on a basis of intelligence tests, of such categories as idiot, feebleminded, sub-normal, average normal, superior and genius, but that within the group of so-called mentally normal human beings, which includes most college students, there still exist rather large differences in intellectual capacity. We all know this to be a fact, but few of us give it sufficient attention; few of us give it sufficient importance as an aid in guiding our practical activities. Now the value of the university's product is, as Terman well says, determined as much by the original quality of the raw material with which it works as it is by salary budget, instructional methods or curriculum. In an abundantly documented recent paper, this active exponent of modern psychology reveals the high significance which an analysis of the intellectual status of the student body of a university might have as a basis for positive action by any university determined to make the best use of its available resources for the advancement of American learning. He shows the positive economy in money, time and mental energy that could be effected by certain radical changes in university administration, and the highly desirable results which would come from these changes in the way of enabling the university to fulfil its highest function in the advancement of learning both through teaching and research. And only by such fulfilment can the nation make the most of its potential mental capacity.

I seem to have wandered somewhat from the particular subject which the title of my paper indicates to be especially mine this afternoon. But all of the things I have talked about have their definite relation to research in the universities. Yet one important phase of this subject I have alluded to by no more than a fortuitous juxtaposition of words. The relation between research and teaching is a subject which alone calls for another and longer paper than all of this present one, which ought to be inflicted on you some time by somebody. This is not the time nor am I the brutal body to do it. But I can not refrain from calling your attention, in my last moment with you, to the additional evidence of the curious and abnormal character of the institution we call university in America, which is afforded by the strange and highly injurious artificial opposition that has been created between research and teaching by the customs and methods of American higher education. Research and teaching are inseparable from, and indispensable to, each other in a real university. An institution which does radically separate or oppose them is not a university, however good and useful some other thing it may be. The University of Minnesota is a university because it is an institution which recognizes the intimate relationship and coincidence of teaching and research. And we may feel assured that under its new president it will continue, and with ever-increasing effectiveness, to fulfil its genuinely university function.

VERNON KELLOGG
THE NATIONAL RESEARCH COUNCIL

THE METRIC SYSTEM IN JAPAN

THE American Metric Association has received from the Decimal Association of London a brief statement by Dr. C. E. Guillaume, director of the International Bureau of Weights and Measures, relating to the progress of the metric system in the Far East. This was written on May 23, 1921, and we have had it translated for the information of the readers of SCIENCE. We have received from official Japanese sources additional information in regard to the Japanese metric law, passed on April 11, 1921, and the program for rendering it effective.

Practically all readers of SCIENCE will be glad to know that the Britten bill, now known as H. R. 10, is being endorsed by national organizations and has a fair chance of pass-

ing. Metric reports are being prepared by the United States Chamber of Commerce and the National Industrial Conference Board. The metric campaign is on in earnest and there should and will be no let up until success is won for North America.

The annual meeting of the American Metric Association for 1921 will be held in Toronto on December 29. In view of the importance of the movement in America, we hope that a large number of the members of the A. A. A. S. will reserve December 29 for the program of the American Metric Association.

Howard Richards, Jr.,
Secretary, American Metric Association

The Obligatory Adoption of the Metric System by the Empire of Japan

A telegram from Mr. Shirio Kikkawa, Director of the Bureau of Weights and Measures in Tokio, brings the news of the passage by the Japanese Parliament of the law rendering the employment of the metric system obligatory. The importance of this event, significant in itself, becomes greater in view of the fact that this settles the supremacy of the metric system in the Far East and also practically in the whole world. In Asia, legislative acts have, during the past few years, paved the way for a greater use of metric units and the governments are now making these acts effective. The work is pushed systematically in such a way as to assure gradual expansion, thus avoiding mistakes and inconveniences.

In Japan the metric system became legal on January 1, 1893, and at the same time the value of the old Japanese units, the shaku and the kwan, were fixed respectively at 10/33 of the meter and at 15/4 of the kilogram. The divisions of these Japanese units were also decimal. Subsequently a series of modifications of this law, and the promulgation of regulations, assured the increased use of the metric measures leading up to the time when their use should become obligatory.

In China the law of August 29, 1908, has given definite values to units which until then were variable according to the localities and the trades. The ch'ih and the liang have been fixed respectively as 32 centimeters and 37.301 grams. The metric equivalents are inscribed in the law; and the subdivisions of these Chinese units are also entirely decimal.

The law approved in 1913 by the Parliament of Pekin prepares for the complete and obligatory adoption of the metric system; a program of preparation and partial adoption is annexed to this law and leads, after ten years, to the obligatory use of the metric system.

Finally, in Siam, a law of 1912 prescribes the obligatory use of the metric system with gradual expansion from one province to another depending on the time required to secure a sufficient number of measuring devices and metric standards.

As can be seen from the preceding paragraphs, in all the Far East, the definite adoption of the metric system is decided in principle; delays in securing the general use of the metric system in the Far East can now only postpone it for a few years.

On the other hand, the House of Representatives of the United States has before it a bill dated April 11, 1921, introduced by Congressman Britten, which will render the use of the metric system obligatory for commercial transactions 10 years after its passage. It is well to note that the adversaries of the reform have heretofore considered it a good argument that the Anglo-Saxon measures were received in China, Japan, and Siam, having almost the same standing as the local measures. The promulgation of the new Japanese law reverses the sense of that argument.

C. E. Guillaume

SCIENTIFIC EVENTS

THE PARIS ACADEMY OF SCIENCES

The *Annuaire* of the Académie des Sciences for 1921 gives as usual a complete list of the members, as well as of the foreign associate members, the correspondents and the "académiciens libres." The annual also gives, as it always does, the names and dates of the successive presidents from the foundation of the Académie des Sciences, as the First Class of the newly organized Institut, on December 27, 1795, to the present time. At the close is an alphabetical "Index Biographique" of all the members and correspondents from 1795 until 1921. This covers nearly 200 pages (pp. 117–314). It mentions a complete list of all the prizes founded by or for the Académie.

The necrology of the Académie for 1920, includes the following members:

M. Armand Gautier, of the Section of Chemis-

try, died at Cannes, July 27, 1920, in his eighty-third year.

M. Jean Jacques Théophile Schloesing, of the Section of Rural Economy, died at Paris, February 8, 1919, in his ninety-fifth year.

M. Yves Delage, of the Section of Anatomy and Zoology, died at Sceaux (dept. Seine) October 7, 1920, at the age of sixty-six years.

M. Adolphe Carnot, Académicien Libre, died June 21, 1920, in his eighty-first year. It is after him that the great radium source, carnotite, has been named.

Of the Foreign Associate Members, the death is announced, in Berlin, of

Simon Schwendener, of Buchs, canton of St. Gall, Switzerland. He was born February 10, 1829, and at the time of his death, May 27, 1920, he was in his ninety-first year.

The following new members were chosen in 1920:

Augustin Mesnager, Section of Mechanics, elected March 1, 1920. Born in Paris June 11, 1862.

Léon Lindet, Rural Economy, chosen March 15, 1920. Born in Paris April 10, 1857.

Maxime Laubeuf, Section of Industry, elected March 22, 1920. Born at Poissy (dept. Seine-et-Oise) November 23, 1864.

Jules Louis Breton, Académicien Libre, elected November 29, 1920. Born at Courrières (dept. Pas-de-Calais) April 1, 1872.

G. F. K.

THE IOWA LAKE SIDE LABORATORY

TRUSTEES for the Iowa Lake Side Laboratory at Lake Okoboji are to acquire a majority interest in the holdings of the stock company which now owns the property. A reorganization of the business control is to be effected, and owners of stock will be solicited to surrender their shares to the trustees in order that the work of the laboratory may be carried on in the best manner possible.

A committee from the University of Iowa Association has been authorized to secure funds for the repairing and general upkeep of the laboratory premises, and an endowment fund of $10,000 will be sought for this purpose. Mrs. F. A. Stromsten, of Iowa City, is chairman of the committee, the other members being A. J. Cox and Mrs. Preston C. Coast, of Iowa City; Dr. F. J. Smith, of Milford; and Fred Pownall, of Des Moines.

President Emeritus Thomas H. Macbride, who has been most active in the interests of the laboratory ever since it was established, has resigned his position on the board of trustees. Dr. Macbride has carried practically the entire burden of responsibility for the financial support of the institution, which has attained enviable distinction in recent years through the quality of work done there and the facilities and resource of material which it affords. His place on the board will be taken by Walter M. Davis, of Iowa City, who becomes custodian of property. Mrs. F. A. Stromsten, of Iowa City, was elected to succeed Euclid Sanders, of Iowa City, who has been in Europe for some time. W. O. Finkbine, of Des Moines, remains as chairman of the board, the other two members being C. F. Kuehnle, of Denison, and J. J. McConnell, of Cedar Rapids.

JENNINGS ANNIVERSARY CELEBRATION

AT the Harvard Commencement of 1896 Herbert Spencer Jennings, now Henry Walters Professor of Zoology in the Johns Hopkins University, received the Ph.D. in zoology. During the present year his students, teachers, colleagues, and friends have joined in a recognition of the twenty-fifth anniversary of his doctorate. A committee consisting of S. O. Mast, chairman, R. W. Hegner, Raymond Pearl, and Ruth Stocking Lynch, secretary, had charge of the arrangements.

The number of contributors was 135, geographically distributed as follows: Baltimore 31, Washington 9, Philadelphia 9, Northeast 25, South 8, Middle West 14, Far West 27, Canada 1, Germany 5, Holland 1, Switzerland 1, Japan 1, Philippines 2, Hawaii 1.

A sufficient sum of money was subscribed to carry through the following projects:

1. A portrait of Professor Jennings, painted by the well-known Philadelphia artist, Mr. Frank B. A. Linton. This portrait was presented to the trustees of the Johns Hopkins University by Professor A. O. Lovejoy at the Commencement exercises this year, and hung

in Gilman Hall, as a permanent memorial of the occasion.

2. An anniversary volume, made up of congratulatory letters to Professor Jennings, together with photographs of the writers, to the end that he personally may have a permanent reminder of the affection and esteem in which he is held by his old students and colleagues. This volume in quarto format is bound in full crushed levant morocco, and contains a large number of letters from all parts of the world.

3. A large silver platter presented to Mrs. Jennings, as a token of the part she has played in her husband's scientific career, and in the lives of his students during their university days.

4. Photographic copies of the portrait, one of which is to be sent to each contributor to the celebration.

On May 28 a subscription dinner was given in Professor Jennings's honor at the Chateau in Baltimore. Some 44 persons were in attendance. Dr. C. B. Davenport presented a remarkably impressive review of Jennings's scientific career and contributions to biology. The anniversary volume was presented. In replying Professor Jennings gave an analysis, at once penetrating and humorous, of the manifold advantages of maturity over youth.

SCIENTIFIC NOTES AND NEWS

Dr. Alexis Carrel, member of the Rockefeller Institute for Medical Research, has been elected a national associate of the Paris Academy of Medicine. Under the rules of the academy there may be only twenty national associates, all of whom have heretofore been residents of France.

Dr. Hideyo Noguchi, of the Rockefeller Institute for Medical Research, has received the honorary degree of doctor of science from Brown University, as well as from Yale University. Dr. Oswald T. Avery, of the institute, received the honorary degree of doctor of science from Colgate University.

The University of Pennsylvania has conferred the degree of Doctor of Laws on Dr. Hobart Amory Hare, professor of materia medica in Jefferson Medical College.

Bowdoin College has conferred the degree of Doctor of Science on Dr. Preston Kyes, professor of preventive medicine in the University of Chicago.

The degree of Doctor of Science has been conferred by Tufts College on Frank William Durkee, professor of chemistry at the college, and on William Henry Nichols, chairman of the Allied Chemical and Dye Corporation.

Honorary degrees of Doctor of Science were conferred on June 15 by Colorado College on Professor S. L. Goodale, professor of metallurgy at the University of Pittsburgh, and on Dr. C. A. Hedblom, of the Mayo Foundation. Both are alumni of the Colorado College.

Dr. John A. Kolmer, professor of pathology and bacteriology in the graduate school of medicine of the University of Pennsylvania, and director of the pathologic laboratories of the Dermatological Research Institute, received the honorary degree of Doctor of Science at Villanova College.

The University of Manchester has conferred the degree of D.Sc. on Dr. C. S. Sherrington, professor of physiology, Oxford, and president of the Royal Society; on Dr. Horace Lamb, formerly Beyer professor of mathematics in the university; and on Sir Ernest Rutherford, formerly professor of physics. The degree of Litt.D. has been conferred on Dr. G. Elliot Smith, formerly professor of anatomy.

According to *Nature* the list of honors conferred on the occasion of the King's birthday includes the following names of men known to the world of science: *Knights:* Professor Arthur Keith, Hunterian professor and conservator of the Royal College of Surgeons; Dr. T. Lewis, hon. consulting physician since April, 1918, to the Ministry of Pensions; Dr. S. Russell-Wells, vice-chancellor of the University of London; Dr. F. Conway Dwyer, ex-president of the College of Surgeons, Ireland; Mr. J. B. Harrison, director and govern-

ment analyst, Department of Science and Agriculture, British Guiana; and Brig.-Gen. D. J. McGavin, director-general of Medical Services in New Zealand. *C.B.:* Mr. Ll. S. Lloyd, assistant secretary to the Department of Scientific and Industrial Research. *K.C.I.E.:* Col. W. H. Willcox, late medical adviser to the Civil Administration in Mesopotamia. *C.I.E.:* Dr. M. N. Banerjee, principal of Carmichael Medical College, Belgatchia, Bengal. *Companion Imperial Service Order:* Mr. G. J. Williams, senior inspector of mines, Mines Department.

PROFESSOR G. F. FERRIS, of Leland Stanford University, California, is spending the summer collecting and studying scale insects in Texas, in cooperation with the Division of Entomology of the Texas Agricultural Experiment Station.

THE British government will devote the sum of 1,000,000*l.* to fostering cotton-growing in the Empire. The money will be placed at the disposal of the British Empire Cotton Growing Corporation, and will be in place of the government's former promise of 50,000*l.* a year for five years to the corporation.

UNIVERSITY AND EDUCATIONAL NEWS

DR. LIVINGSTON FARRAND, chairman of the executive committee of the Red Cross, formerly adjunct professor of psychology and professor of anthropology at Columbia University and president of the University of Colorado, has been elected president of Cornell University.

DR. FRANK PIERREPONT GRAVES, dean of the school of education of the University of Pennsylvania, has been appointed commissioner of education of the state of New York and president of the University of the State of New York.

DR. P. J. HANZLIK, of the medical school of Western Reserve University, has been appointed professor of pharmacology in the Stanford University Medical School to succeed Professor A. C. Crawford, who died recently.

DR. W. H. RODEBUSH, who has been for the past year a research fellow of the National Research Council at the University of California, has been appointed associate professor of physical chemistry at the University of Illinois.

GEORGE M. WHEELER, PH.D. (1921), Bussey Institution, has been appointed instructor in entomology, and William E. Greenleaf, instructor in zoology, in the zoology department of Syracuse University.

DR. R. R. GATES has been appointed to the university chair of botany tenable at King's College, University of London, in succession to Professor W. B. Bottomley. He was appointed university reader in botany at that college in 1919, and has since that date been in charge of the department in the absence of Professor Bottomley.

DISCUSSION AND CORRESPONDENCE

THE CANNONBALL LANCE FORMATION

TO THE EDITOR OF SCIENCE: In reviewing Stanton's memoir on the Cannonball Lance formation, Dr. Schuchert has advocated drawing the line between Cretaceous and Tertiary at the base of the Wasatch. He has referred to the vertebrate evidence as supporting this view, and as recent researches have considerably clarified and extended this evidence, a brief summary of its present status may be of some aid toward harmonizing the existing conflict of opinion.

The position of these border-line formations has been in dispute not merely for a number of years, as Dr. Knowlton remarks, but ever since they were first discovered. A Cretaceous vertebrate fauna was found associated with a Tertiary flora. Vertebrate palæontologists and palæobotanists took opposite sides; the stratigraphic geologists were divided, and the relations with the marine succession, European standard, theories of diastrophism, etc., have been invoked by both sides for a decision. This discrepancy has been maintained and confirmed by all subsequent work. It should be recognized as the fundamental difficulty. It does not help matters to misrepresent or ignore any part of the evidence, and if Dr. Cross's references to the vertebrate evidence fairly reflect the way in which the U.

S. Geological Survey "considered all available evidence" it is clear that its weight and tenor was not correctly understood.

When the subject was discussed by the Palæontological Society in 1913 I presented a paper outlining the vertebrate evidence, especially with regard to the Paleocene faunas.[1] Subsequent researches by Brown, Lambe, Osborn and Parks on the Alberta dinosaurs, by Gilmore on the New Mexican reptiles, by Granger and myself on Paleocene and Eocene mammals, by Stehlin, Teilhard and Schlosser on the Eocene and Paleocene mammals of Europe, by Smith Woodward and myself on the Cretaceous mammals of Alberta, confirm the correlations and conclusions presented in that paper, but strengthen certain views which were then rather suggested than advocated.

1. The Lance fauna is wholly Cretaceous in character. It is entirely a continuation and specialization of the Judith (late Cretaceous) without any new elements, but the amount of evolutionary change in the many phyla that have now been traced through Judith, Edmonton and Lance shows that it is considerably later in time.

2. The earliest placental mammals appear in the Puerco "Lower Paleocene" which may be as old as the Lance or older, although usually regarded as later. The Torrejon and Fort Union faunas, Upper Paleocene, are not much later than the Lance, and the phyletic evolution indicates that they are considerably later than the Puerco. The Tiffany and Cernaysian faunas show a still later stage of the Paleocene faunas.

3. The Paleocene placentals are of primitive and archaic aspect. Although some of their phyla survive into the Eocene, they are as a whole not nearly related to the characteristic and dominant Tertiary Mammalia, and much more primitive. The metatherian mammals (multituberculates and marsupials), a minor but considerable element in the Paleocene faunas, are of distinctly Mesozoic aspect and closely related to those of the Judith and Lance. The reptiles are all Cretaceous families continued from the Judith.

4. The true Tertiary mammal fauna appears suddenly at or near the base of the Wasatch, and in the Sparnacian of Europe (London Clay, etc.). It is a new fauna, identical in these two far distant regions, and consists in the main of the modern orders of mammals, which now appear for the first time and evolve through the course of the Tertiary into their present diversity and specialization. The two most important families of Tertiary and modern chelonians (terrapins and tortoises) appear at the same time.

5. The great faunal break lies at the end of the Paleocene, with the incoming of the Cenozoic vertebrates at or near the base of the Wasatch. The European standard has drawn the line above the great chalk formations and below the Thanetian (Cernaysian). The Judith corresponds to the Upper Senonian of Europe, but is older than the Maestrichtian and Danian divisions of the chalk, unquestionably Cretaceous, aside from certain formations of disputed age grouped as Montien. The end of the unquestioned Cretaceous in Western Europe is then considerably later than the Judith, perhaps as late as the Lance or later. Its precise correlation can best be made through comparisons of the marine Cannonball phase of the Lance formation with the Danian, etc. On the other hand the Tertiary as generally recognized in Western Europe begins at least with the Thanetian, containing the Cernaysian fauna, uppermost Paleocene, equivalent to the Tiffany zone at the base of the Wasatch in the San Juan basin. It is therefore a little below the great migrational break indicated by the vertebrate faunas.

There are two criteria generally used in faunal classifications, the extinction of ancient types and the first appearance of new groups. The latter appears to me the more logical and practical. By this standard the Wasatch Sparnacian fauna of the London Clay, etc., is the introduction of the distinctively modern or Cenozoic life, the preceding faunas, even including the Paleocene placentals, being essentially the last stages of Mesozoic life.

This division is not supported by the palæobotanists. Their Cenophytic era, it is well recognized, begins with the upper Cretaceous

[1] *Bull. Geol. Soc. Am.*, XXV., pp. 381–402 Sept. 15, 1914.

(Dakota, etc.); they find a sharp floral break between Judith and Lance at a point where no break occurs in the vertebrate fauna; and so far as I understand no serious break between Paleocene and Eocene. I can hardly venture an opinion as to where the majority of invertebrate palæontologists would draw the line, if based wholly on invertebrate data; in practise most of them draw it at the summit of the chalk succession of western Europe.

The great stratigraphic break asserted by some stratigraphers to exist everywhere at the base of the Tertiary is denied by others of no less ability and experience, and its universality and importance seem to have been much exaggerated.

Is it not possible, where the evidence is thus conflicting, to adopt a compromise by mutual concession? It appears to me that the compromise indicated by Schuchert has the best elements for universal acceptance. It is in accord with the historic and universal European usage, including the Thanetian in the Tertiary, but none of the chalk succession. It conforms to the insistence of the palæobotanists that the Lance and Fort Union should be kept together. It gives a satisfactory practical base for the stratigrapher in the widespread and characteristic Wasatch formations. It places all the dinosaur formations and the bulk of the "Paleocene" faunas in the Cretaceous where the former certainly and the latter in my opinion properly belong; but the uppermost Paleocene faunas are placed in the Tertiary. The replacement of the Cretaceous by the Tertiary vertebrate fauna would thus be a little later, of the Upper Cretaceous by the Tertiary flora a little earlier than the line agreed upon.

W. D. MATTHEW

NEWCOMB ON EXTRA-MUNDANE LIFE

TO THE EDITOR OF SCIENCE: As one long interested in the subject matter covered by the inquiry of Professor Clark, published in SCIENCE of May 13, I have read with some care Newcomb's essay to which Professor Campbell refers, in the same issue of SCIENCE. While this essay may be presumed to represent an opinion at some time entertained by its distinguished author, an opinion that merits respect, it seems wholly unresponsive to the request for evidence upon which such an opinion may be based. The author expressly admits that "scientifically we have no light upon the question and therefore no positive grounds for reaching a conclusion." In another place, *Popular Astronomy*, ed. 1890, p. 528, he amplifies as follows:

> The spirit of modern science is wholly adverse to speculation on questions for the solution of which no scientific evidence is attainable, and the common answer of astronomers to all questions respecting life in other worlds would be that they knew no more on the subject than any one else and having no data to reason from, had not even an opinion to express.

It is probable that few astronomers will dissent from either of these statements. Most of them, Newcomb included, will concur in the statement that of the hundred or more millions of celestial bodies known to exist it may be shown with a high degree of probability that, barring our two neighbors, Mars and Venus, no one of them is suited to be the abode of animate beings. As to the numerous worlds alleged to be the abode of life, Newcomb in his essay raises the question: "But where are we to look for these worlds?" and replies to it: "This no man can tell." Nevertheless, as quoted by Professor Campbell, he goes on to say:

> It is perfectly reasonable to suppose that beings not only animated but endowed with reason inhabit countless worlds in space.

A major premise upon which this conclusion might rest would seem to be: We may reasonably suppose anything that does not admit of disproof. In the bald form here stated this premise would doubtless be rejected by those who believe in the plurality of abodes for animate intelligence, but without some appropriate equivalent for it there seems to be a hiatus between the conclusion above set forth and the facts that constitute its minor premise. Possibly Newcomb's own words anent this subject matter, *loc. cit.*, p. 531, may be a less objectionable formula:

Here we may give free rein to our imagination with the moral certainty that science will supply nothing tending either to prove or to disprove any of its fancies.

In this connection one is reminded of a famous apothegm,

Faith is the substance of things hoped for, the evidence of things not seen.

George C. Comstock

QUOTATIONS

COOPERATIVE INDEXING OF SCIENTIFIC LITERATURE

We have shown that the core or *umbra* of a subject is comprised in a body of homogeneous literature which unquestionably can best be dealt with by its representative professional society, but that outside this core there exists a *penumbra* of relevant matter dispersed through a literature of gradually increasing irrelevance, with the result that the recovery of the relevant matter can be effected economically only by cooperative effort. The solution, therefore, would appear to be to bring into existence a central bureau which should deal solely with the indexing of periodicals of the non-homogeneous character—and in the first stages of its work, with a restricted list of periodicals assigned to it by the contributory bodies. These bodies would receive from the central bureau entries from the periodicals examined corresponding to their specified requirements. But as the professional abstracts became more fully representative of progress in their respective fields the need for the publication of the corresponding indexes would tend to disappear. The institution, therefore, of a central bureau would ultimately make for economy in all branches of science in which the publication of abstracts is admittedly indispensable.

So far as science is concerned, it will probably be found that the simplest and most effective method for obtaining the necessary index slips would be to invite the Central Bureau of the "International Catalogue of Scientific Literature" to provide them. Indeed, the possibility of cooperation between the "International Catalogue" and the abstracting journals was one of the subjects considered at the conference held last September. Any such arrangement would probably begin with the year 1921, and, as a preliminary, the "International Catalogue" should be brought up to date by the publication of volumes for 1915–20.—*Nature.*

SPECIAL ARTICLES

THE MOTIONS OF THE PLANETS AND THE RELATIVITY THEORY

Constant reference is made to the motion of Mercury about the sun and to the supposed fact that this motion can not be explained by the Newtonian law of gravitation. This current idea is far from correct: the motion of Mercury can be accounted for fully as well, if not far better, by the Newtonian law than by the Einstein law. The difficulty, which has faced mathematical astronomers for many years, is not how to account for the motion of Mercury, but how to account for that motion without introducing complications in the motions of the other planets.

In 1895 Newcomb[1] showed clearly that the motion of Mercury can be fully accounted for, under the Newtonian law, by one of several possible distributions of matter in the immediate vicinity of the sun and the inner planets. He, however, discarded each such possible explanation of the motion of Mercury because of the difficulties encountered in explaining, at the same time, the motions of the other planets. Each possible explanation of the motion of Mercury introduced a new complication somewhere else in the system.

Now identically the same difficulty is encountered by Einstein. His formulas account for the motion of Mercury, but fail to account for the motion of Mars, and introduce a further complication in the motion of Venus. The supposed explanation of the motion of Mercury by the Einstein formulas has been stressed, but the attendant difficulties in the motions of the other planets have been glossed

1 "The elements of the four inner planets and the fundamental constants of astronomy," by Simon Newcomb.

TABLE I

Secular Motions of the Elements of the Four Inner Planets

	Observed	Computed	Difference	Per Cent.
PERIHELIA:				
Mercury	+ 579.2″	+ 537.6″	+41.6″ ± 1.4″	+ 7.2%
Venus	+ 42.4	+ 49.7	− 7.3 ±22.3	−17.2
Earth	+1161.5	+1155.6	+ 5.9 ± 5.6	+ 0.5
Mars	+1605.9	+1597.8	+ 8.1 ± 2.6	+ 0.5
INCLINATIONS:				
Mercury	+ 7.14″	+ 6.76″	+ 0.38″± 0.54″	+ 5.3%
Venus	+ 3.87	+ 3.49	+ 0.38 ± 0.22	+ 9.8
Mars	− 2.26	− 2.25	− 0.01 ± 0.14	− 0.4
NODES:				
Mercury	− 753.0″	− 758.1″	+ 5.1″ ± 2.8″	+ 6.8%
Venus	−1780.7	−1790.9	+10.2 ± 2.0	+ 0.6
Mars	−2248.9	−2249.8	+ 0.9 ± 4.6	+ 0.0

over by those who accept the relativity theory as proved.

In order to understand fully this question of the motion of Mercury and the difficulties of finding a satisfactory explanation, reference should be had to the secular motions of the elements of the planets, as determined by Newcomb. These motions are given in Table I.

The first column in the above table gives the actual motions in one century as determined from observations of the actual planets; the second column gives the corresponding motions as calculated by the formulas of celestial mechanics, deduced from the Newtonian law of gravitation. It is, however, well known to every mathematical astronomer that these calculations are not complete; that they do not take fully and completely into account all of the bodies of the solar system. In the theories and formulas upon which these calculations depend, the sun has been considered as a perfect sphere and all space between the sun and the various planets as free from all gravitational matter. These are necessary mathematical simplifications; without them the equations of motion would be impossible of solution. These simplifications approximate very closely to the truth and the results obtained by their use very closely represent the motions of the planets, but they are *approximations* and it, therefore, necessarily follows that the results do not accurately represent the actual motions.

The column of differences contains the unexplained portions of the motions of the planets, together with the "probable error" as determined by Newcomb. That is, in one century the perihelion of Mercury moves 41.6″ of arc more than the *approximate calculations* indicate it should; whilst that of Venus does not move quite as swiftly as these computations would lead one to expect. These unexplained portions of the motions are the so-called "discordances" or "discrepancies." That of the perihelion of Mercury is especially well known and has figured prominently in all attempts to prove false the law of Newton. The perihelia of Venus and Mars show large discrepancies, as do also the nodes of both Mercury and Venus.

The probable errors give some idea as to the relative accuracy of the various determinations, but it must be remembered that the assignment of these probable errors is very largely a matter of judgment, and that these values may have been over- or underestimated. In every step of the long and complicated computations an estimate, rather than an exact calculation, has to be made as to the value of the probable error, and the final value, as given in the table, thus depends upon many separate estimations or judgments.

It is known to every astronomer that the assumptions, upon which are based the simplifications used in the calculations, are not true. Neither the sun nor any one of the planets is a perfect sphere. The sun-spots, which

can be seen with an ordinary small telescope, show that the sun is not of uniform shape and density. While exact measurements of the shape of the sun are extremely difficult to make, yet every series of measures, heretofore made, show distinct departure from a true spherical form. The sun is not a sphere.

Passing outward from the sun itself one finds the corona. At times of eclipse this halo, or brilliant crown, about the sun can be seen by the unaided eye. It has been sketched many times; it has been photographed times without number. Its presence proves the sun to be surrounded by an envelope of matter of irregular shape and of vast size. This envelope is in general lens-shaped and it extends far out beyound the orbit of the earth. On clear dark nights the extreme outer portions of it can be seen after sunset as a faint glow in the western sky,—a glow that is well known under the name of the zodiacal light.

While matter is thus known to exist in the vicinity of the sun and the inner planets, yet its effect upon the motions of these planets cannot be accurately calculated. Until its distribution is fully known, its effect can not be reduced to figures. It is perfectly clear that the figure, 537.6″ per century, does not accurately represent the motion of Mercury's perihelion under the Newtonian law; but, in the present state of our knowledge as to the solar envelope, it is impossible to correct definitely this figure and to state finally what the true figure should be.

The whole question of the effect of this matter upon the motions of the planets has been made the subject of several recent investigations, notably by Jeffreys and Seeliger.[2] As the actual distribution of this matter is unknown, the problem is attacked in reverse: that is, from the discordances is found a general distribution of matter, which will account for the motions, and this calculated distribution is then compared with the known facts. This procedure is analogous to the method by which the planet Neptune was discovered.

The matter in the immediate vicinity of the sun would tend to group itself about a plane somewhere near that of the solar equator, or that of the orbit of Mercury; whilst matter at a considerable distance from the sun would tend more towards the invariable plane of the planetary system, which is nearly the same as that of the orbit of Jupiter. Further the density of the matter will decrease as the distance from the sun increases. This general distribution can be approximated to by assuming the whole mass to be made up of ellipsoids of revolution, each ellipsoid to be of uniform density, but the larger ones to be of much less density than the inner ones.

An ellipsoid, or ring, of matter wholly within the orbit of a planet will give a direct motion to the perihelion. But if the orbit actually lies in the matter composing such ellipsoid, then the effect is the opposite and the motion of the perihelion will be retrograde. This, of course, upon the assumption that the density is uniform throughout; if the density is much greater in the central portions of the ellipsoid, then the retrograde effect of the outer portion may be overcome and the total effect upon the perihelion may be direct, but the motion will be less than that due to the central portion alone. By adjusting the rate at which the density is assumed to decrease, any motion of the perihelion, direct or retrograde, within limits can be obtained. To changes in the density of the envelope surrounding the sun may thus be attributed the discordant motions of the perihelia of the four inner planets, and especially the retrograde discrepancy in the motion of Venus.

The entire mass of matter, which is known to exist, may for the purposes of computation be considered as made up of three ellipsoids, or as showing two abrupt changes in density. The small central dense portion lies wholly within the orbit of Mercury, the intermediate portion wholly within the orbit of the earth,

[2] "The secular perturbations of the four inner planets," by Harold Jeffreys, *Month. Notices, R. A. S.*, Vol. LXXVII., p. 112.

"Das Zodiakallicht und die emperischen glieder in der bewegung der planaten," by Seeliger.

De sitter, *Observatory*, Vol. XXXVI., 1913.

TABLE II

Final Discordances in the Secular Motions of the Elements of the Four Inner Planets

	Amounts to Account for Newcomb	Amounts Accounted for by			Final Discordances	
		Einstein	Seeliger	Poor	Einstein	Poor
PERIHELIA:						
Mercury	+41.6″	+42.9″	+41.7″	+41.6″	− 1.3″	+0.9″
Venus	− 7.3	+ 8.6	+ 7.3	− 7.5	−15.9	+0.2
Earth	+ 5.9	+ 3.8	+ 4.1	+ 5.9	+ 2.1	+ 0
Mars	+ 8.1	+ 1.3	+ 6.4	+ 6.9	+ 6.8	+1.2
INCLINATIONS:						
Mercury	+ 0.38″	0	0	+ 0.37″	+ 0.38″	+0.01″
Venus	+ 0.38	0	0	+ 0.45	+ 0.38	−0.07
Mars	− 0.01	0	0	+ 0.12	− 0.01	−0.13
NODES:						
Mercury	+ 5.1″	0	+ 5.4″	+ 4.9″	+ 5.1″	+0.2″
Venus	+10.2	0	+10.0	+ 9.1	+10.2	+1.1
Mars	+ 0.9	0	+ 7.2	+ 4.3	+ 0.9	−3.4

and the outer, or less dense, portion extends beyond the orbit of the earth nearly to that of Mars. The effect of each ellipsoid upon the perihelia, the nodes, and the inclinations of the planets can be found by simple formulas of celestial mechanics, and the positions and densities of those ellipsoids, which will best account for all the motions, can be determined. No distribution can be found that will rigorously satisfy all the motions, but the positions and densities of three ellipsoids can be found which will approximately satisfy all the equations and practically account for all the discordances in the motions of the planets.

The table given above shows with what a high degree of accuracy the motions of the planets can be accounted for under the action of this widely scattered matter. For purposes of comparison the Einstein motion is also given.

The relative probabilities of two theories, or two solutions of a problem, are usually determined from the final differences, or residuals, as these differences are called. That solution is deemed the more probable which makes the sum of the squares of the residuals the smaller. If this test be applied to the residuals as given in the above table, the results are:

Einstein theory	436
Solar envelope, Seeliger	259
Solar envelope, Poor	14

And these clearly indicate how very much more probable is the explanation of the motions of the planets as due to the presence of matter in space, than as due to the hypotheses of Einstein.

Einstein and his followers have cited the motions of the planets as proof of the truth of his hypotheses. The evidence does not sustain this—his hypotheses and formulas are neither *sufficient* nor *necessary* to explain the discordances in these motions. They are not *sufficient*, for they account for only one among the numerous discordances—that of the perihelion of Mercury; they are not *necessary* for all the discordances, including that of Mercury, can readily be accounted for by the action, under the Newtonian law, of matter known to be in the immediate vicinity of the sun and the planets.

It is, however, possible that the Einstein hypotheses be true, and that the discordant motions of the planets result from a combination of the Einstein motions and the effect of the widely distributed matter in space. Just as a definite distribution of matter can be found which will explain the discordances given by Newcomb, so also another and different distribution can be found that will more or less fully account for the discordances remaining after applying the Einstein effects. But it is clear that the relativity theory alone is not *sufficient* to explain the motions of the planets.

verba et praeterea ?

Thus the motions of the planets do not prove the *truth* of the Einstein theory, nor, on the other hand, do they prove its *falsity*. While these motions can be accounted for by a certain distribution of matter in the solar envelope, it has not yet been established by observation that the matter is actually distributed through space in the required way. The presence of the matter is unquestioned; its distribution is still a problem of observational astronomy. In the present state of our knowledge regarding the distribution of this matter throughout space, the motions of the planets do not and can not furnish a definite answer to the question as to the validity of the Einstein hypotheses of relativity.

Charles Lane Poor
Columbia University,
April, 1921

THE AMERICAN CHEMICAL SOCIETY

(*Continued*)

DIVISION OF ORGANIC CHEMISTRY

Rodger Adams, *chairman*
H. T. Clarke, *secretary*

Arsenated benzophenone and its derivatives: W. Lee Lewis and H. C. Cheetham. Benzarsonic acid is best prepared by reduction of p-nitrobenzoic and arsenation by means of Bart's reaction. With phosphorus trichloride and pentachloride, dichloro-p-arsinobenzoyl-chloride results. By means of the Friedel and Crafts reaction, benzophenone-p-arsenious oxide, arsenious acid, and arsonic acid are formed. The similar derivatives of 4-methyl benzophenone-p-arsenious oxide, 4-methoxy benzophenone-p-arsenious oxide, and 4-phenoxybenzophenone-p-arsenious oxide have been prepared. Their nitro compounds, oximes, and isomers are being studied. With arsanilic acid, dichloro-p-arsinobenzoylchloride gives the di-arsenated benzanilide. The further reactions of dichloro-p-arsinobenzoylchloride with hydrocarbons, ethers, phenols, and various amino- and hydroxy-compounds are being studied.

6 chlorophenalphanapthazarsine and some of its derivatives: W. Lee Lewis and C. S. Hamilton. 6 chlorophenalphanapthazarsine is prepared by heating phenyl alpha naphthylamine with arsenic trichloride. 6 cholorophenalphanapthazarsine with hydrogen peroxide gives phenalphanapthazarsinic acid. The sodium salt of this acid has been prepared. A series of compounds, 6 methoxyphenalphanapthazarsine, 6 ethoxyphenalphanapthazarsine, 6 propoxyphenalphanapthazarsine, 6 butoxyphenalphanapthazarsine, 6 phenoxyphenalphanapthazarsine are prepared by treating 6 chlorophenalphanapthazarsine dissolved in xylene with the corresponding sodium alcoholate. 6 bromophenalphanapthazarsine is prepared by refluxing 6 phenalphanapthazarsine oxide or 6 phenoxyphenalphanapthazarsine with hydrobromic acid. 6 phenalphanapthazarsine oxide is prepared from 6 chlorophenalphanapthazarsine by heating with silver oxide.

Condensation reactions with benzyl cyanide: Fred W. Upson and T. J. Thompson. Several phenyl alkyl succinic nitrils have been made by condensing benzyl cyanide either (1) with the cyanhydrine of the aliphatic aldehyde in the presence of sodium methylate, or (2) with the α brom ester of the fatty acid in the presence of sod-amide in ether suspension. Saponification of the resulting condensation products has given substituted succinic acids. The following have been made by method No. 2: methyl phenyl succinic acid, m.p. 185°; ethyl phenyl succinic acid, m.p. 194°; N. propyl phenyl succinic acid, m.p. 214°; and the following have been made by both methods: Iso-propyl phenyl succinic acid, m.p. 178°; iso-butyl phenyl succinic acid, m.p. 186°. The nitrils of the higher members can be saponified only under pressure. Some evidence has been obtained for the formula $C_6H_5—\overset{H}{C}=C=N—Na$ for the sodium salt of benzyl cyanide.

Derivatives of trihalogen tertiary-butyl alcohols. IV. The benzoic acid ester of tribromo-tertiary butyl alcohol or brometone benzoic acid ester: T. B. Aldrich. The benzoyl ester of tribromo tertiary-butyl alcohol, $C_6H_5CO.OC—C_3H_6Br_3$, is prepared most conveniently by mixing molecular quantities of benzoyl chloride and preferably anhydrous tri-bromo tertiary butyl alcohol and heating on the steam bath until hydrogen bromide ceases to be given off. The ester is purified by heating on the steam bath with 5–10 per cent. caustic soda solution, washing with water, and finally recrystallizing from alcohol. It crystallizes in the monoclinic system and melts at 89–90°. It is readily soluble in the organic solvents, but insoluble in water. It is not so readily saponified as the aliphatic esters of either chloretone or bromotone. It is practically non-volatile at either incubator or room temperature, and is not volatile with steam to any extent. In general its properties are the same as the corresponding ester of trichloro-

tertiary butyl alcohol. Analytical data show its composition to be $C_{11}H_{11}O_2Br_3$.

Trihalogen-methyl reactions. IV. Tetrachlorosuccinic acid: HOWARD WATERS DOUGHTY and BENJAMIN FREEMAN. Tetrachlorosuccinic acid is formed by the action of trichloroacetic acid on copper dust in benzene solution. It is a hygroscopic solid which is not very stable, being easily hydrolyzed. The aniline salt (m. p. 149°) and ethyl ester (b. p. 156°/13 mm.) are described. Ammonium trichloroacetate reacts vigorously with copper in aqueous ammonia, losing two atoms of chlorine per molecule. This action also takes place still more vigorously with cadmium and zinc, but not with silver.

Spiro-pyrimidines. II. Cyclohexane-1, 5-spiro-pyrimidines: ARTHUR W. DOX and LESTER YODER. Cyclohexane-1, 1-dicarboxylic ester, prepared by condensation of 1, 5-dibromopentane with ethyl malonate, condenses with urea and with guanidine to form cyclohexane-1, 5-spiro-pyrimidines. These products are very similar in their properties to the corresponding cyclobutane-1, 5-spiro-pyrimidines previously described by the writers. In certain respects they resemble also the dialkylbarbituric acids, but differ from the latter in having a carbon atom common to two rings.

Spiro-pyrimidines. III. Cyclopropane-1, 5-spiro-pyrimidines: ARTHUR W. DOX and LESTER YODER. Cyclopropane-1, 1-dicarboxylic ester, prepared by condensation of ethylene bromide and ethyl malonate, might be expected to condense with urea and substituted ureas and yield spiro-pyrimidines analogous to those obtained from cyclobutane- and cyclohexane-1, 1-dicarboxylic esters. Condensations with urea and with guanidine were readily effected, but the products were invariably amorphous, insoluble substances of great stability. Analyses showed the same percentages of nitrogen as those calculated for the simple spiro-derivatives. It is probable that the cyclopropane ring opens and two or three molecules unite to form a cyclobutane or cyclohexane nucleus with two or three barbituric acid groups attached. The polymer obtained from urea could not be hydrolyzed by long boiling with concentrated hydrochloric acid. Hydrolysis by means of sodium hydroxide gave an acid which lost carbon dioxide on heating, with formation of a crystalline acid, of melting point 151° and neutralization equivalent 86.

Pyrimidines from dialkylmalonic esters and benzamidine: ARTHUR W. DOX and LESTER YODER. In the presence of sodium ethylate at 70–75°, dialkylmalonic esters condense with benzamidine to form derivatives of tetrahydropyrimidine. The dialkyl-malonic esters used were the dimethyl, diethyl, dibutyl, diisoamyl, and dibenzyl. These all gave white products. The corresponding monoalkyl derivatives were bright yellow. The latter may therefore be regarded as possessing the tautomeric enol structure.

An electrochemical study of certain reversible reductions: J. B. CONANT and H. M. KAHN.

The reactivity of the chlorine atom in the nitrobenzyl chlorides: J. B. CONANT and S. S. NEGUS.

The 1.4-addition of phosphenyl chloride: J. B. CONANT and S. M. POLLACK.

A comparative study of ring stability: NAO UYEI and OLIVER KAMM.

Investigation of isomerism in the diphenyl series: J. H. WALDO, C. S. PALMER and O. KAMM. Fixation of the benzene nuclei in diphenyl derivatives according to the Kaufler-Cain theory leads to the possibility of optical isomerism in the case of certain diphenyl derivatives. Salts of diphenyl-O-carboxylic acid with optically active bases were subjected to crystallization but there was found no conclusive evidence of this type of isomerism. The investigation has been extended also to benzidine derivatives, particularly to benzidine disulfonic acid. The negative results obtained fail to support the recently proposed theories concerning the structure of the diphenyl derivatives.

The action of hydrogen sulphide upon tri-nitro-toluene: F. J. MOORE and E. H. HUNTRESS. In this reaction Cohen and Dakin observed the formation 2-6-di-nitro 4- tolyl hydroxylamine which when boiled with hydrochloric acid yielded a colorless solid which they assumed to be the amine, but later denied this without further characterizing it. They subsequently obtained the true amine by the addition of hydriodic acid. The observation that the above hydroxylamine yields this same compound when treated with silver nitrate raised the suspicion that it might be the azoxy compound. This accounts for the oxygen which ought to have been evolved in the original equation of Cohen and Dakin, for we have shown that 2-6 dinitro-4-tolyamine is formed at the same time.

The constitution of the secondary product in the sulphonation of cinnamic acid: F. J. MOORE and RUTH M. THOMAS. The principal product of the reaction is para-sulpho-cinnamic acid. With this is formed an isomer whose barium salt is much more soluble. This has hitherto been variously de-

scribed as an ortho and a meta compound. It has been supposed to be different from the meta compound prepared by Kaffka from m-sulpho benzaldehyde by the Perkin synthesis. We have shown that it is identical, and also that on oxidation with permanganate it yields meta sulpho benzoic acid. It can be easily characterized by the melting points of its aniline and toluidine salts.

Separation of aromatic primary and secondary amines: I. N. Hultman and H. T. Clarke. The separation of primary and secondary amines can in most cases be carried out satisfactorily by taking advantage of the solubility in alkali of the benzenesulfonyl or toluenesulfonyl derivatives of the primary amines. The recovery of the base from such derivatives is however apt to be troublesome, and in certain instances difficulties are met with in the alkali treatment owing to the ease with which the sodium salts are hydrolyzed. Thus they can be extracted from their solution in alkali merely by shaking with ether; and in cases where only a small quantity of primary base is present, the sulfonyl derivative of the secondary amine acts in the same way as an extracting solvent. This was found to be the case particularly with p-toluidine and its monomethyl derivative. This mixture moreover can not be separated by treatment with zinc chloride solution, as is possible in the case of aniline and methylaniline, since methyl-p-toluidine appears to form an insoluble zinci-chloride. Advantage can therefore be taken of the observation that primary aromatic amines, on heating to 160–180° with urea are converted into symmetrical diarylureas, while secondary bases, typified by methylaniline, do not react at all with urea. It is thus merely necessary to heat the mixed bases with urea in slight excess over the calculated amount, and treat the product with dilute acid.

Potassium derivatives of the alkyl amines: Edward C. Franklin. By the action of metallic potassium on methyl amine, ethyl amine and dimethyl amine in the presence of platinum black, the compounds represented by the formulas CH_3NHK, C_2H_5NK and $(CH_3)_2NK$ have been prepared.

The existence and reactions of positive halogens attached to carbon in aromatic compounds: Ben H. Nicolet. W. A. Noyes, J. Stieglitz, L. W. Jones and others have interpreted and contributed a large amount of data on the positive nature of halogen attached to nitrogen. Howell and Noyes have shown the I of C_2I_2 to be positive. It is now shown that a number of compounds, most typically p-iodoaniline and 3-iodo 4-toluidine, can be hydrolyzed with acids in such a way that the halogen is replaced by hydrogen, and that the halogen resubstitutes to give di-halogenated products—two proofs of positivity. Such halogens appear to be readily removed (replaced by hydrogen) by heating with $SnCl_2$ and HCl.

Diphenyl-β-naphthylemethyl: M. Gomberg and F. W. Sullivan, Jr. Diphenyl-β-naphthylmethyl was prepared and found to exhibit the characteristic chemical reactions and physical properties of free radicals. Determinations by the cryoscopic method showed that dissociation increased with rising temperature and with dilution. Contrary to the theory that the color of free radical solutions is due to dissociation of the dimolecular to the monomolecular form, investigation by colorimetric methods showed that the increase in color intensity was quite independent of changes in dissociation. It was also found that not more than one third of the monomolecular free radical was in the colored form. From these facts we believe that the color of solutions of free radicals is best explained on the assumption that the dimolecular form dissociates to the mono-molecular form in which equilibrium exists betweeen the colorless benzenoid and the colored quinonoid tautometer.

Contribution to the structure of benzidine: Formation of rings through the m and p positions of benzene: Roger Adams and W. C. Wilson. Benzidine condenses readily with aromatic dialdehydes or ketones of the type represented by terephthalaldehyde and resodiacetophenone,. to give condensation products which analyze for one molecule of benzidine plus one molecule of dialdehyde with the elimination of two molecules of water. It is barely possible that these substances consist of two molecules of benzidine with two molecules of dialdehyde, with the elimination of four molecules of water. The substances are too insoluble to allow the molecular weight to be obtained. If these substances are of the simpler type, it is difficult to see how Kaufler's formula for benzidine can be accepted, since his structure assumes that the amine groups are as close together as the amine groups in ophenylenediamine. Benzidine also condenses with the monazine of terephthalaldehyde to produce a product which analyzes for one molecule of each with the elimination of two molecules of water. It is apparent

that ring structures containing a very large number of atoms and connecting the meta and para positions in the benzene nucleus are readily prepared.

The preparation of dihydrobenzene and some of its derivatives: E. C. KENDALL and A. E. OSTERBERG. The use of certain sulfonic acids for the dihydration of quinite or of tetrahydrophenol. The yield of dihydrobenzene from quinite by catalytic action of phenolsulfonic acid is practically quantitative. Dihydrobenzene Δ 1:4 adds hydrochlorous acid and halogens.

Stability of the C-Hg linkage in mercury derivatives of anisole and phenetole: EDMUND B. MIDDLETON and F. C. WHITMORE. The stability of this linkage resembles that of the corresponding linkage in acetylated mercury derivatives of phenol. While the C-Hg linkage in mercury compounds containing a phenolic hydroxyl is broken quantitatively by inorganic iodides and similar reagents the C-Hg linkage in the anisole and phenetole compounds is stable to these reagents. The ortho anisyl and phenetyl mercuric halides give the corresponding R_2Hg compounds. The para compounds form the iodides which remain unchanged. Potassium sulfocyanate gives the same results as the iodides with mercurated phenols, their acetyl derivatives, and mercurated anisoles and phenetoles.

Preparation of mercury ditolyl from tolymercuric chloride: L. FRANCES HOWE and F. C. WHITMORE. Tolylmercuric chloride prepared from toluene sulfinic acid obtained from p-toluene sulfone chloride was treated with the reagents usually used for changing compounds of the type RHgX to those of the type R_2Hg. Metallic copper in alcohol and sodium stannite in water gave very poor yields. Sodium in xylene, aqueous sodium sulfide, and aqueous sodium thiosulfate gave fair yields. Potassium iodide gave an almost quantitative yield. A new reagent for this purpose, potassium sulfocyanate, gave almost as good a yield. Using the sulfone chloride obtained from saccharine manufacture this method is the most convenient for making a mercury diaryl.

Organic compounds prepared from ortho-chloromercuribenzoyl chloride: F. C. WHITMORE and EDMUND B. MIDDLETON. *Preliminary paper.* The acid chloride obtained by the action of thionyl chloride on the anhydride of ortho-hydroxymercuribenzoic acid reacts with alcohols and amino compounds giving mercurated benzoic esters and amides. Compounds have been prepared from methyl alcohol, ammonia, aniline, and p-aminobenzoic acid. These compounds are too insoluble for therapeutic use. The action of more complex alcohols and amino compounds is being studied.

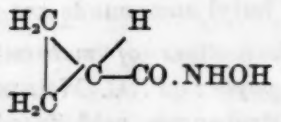
Mercury compounds of normal-butyl: RUTH WALKER and F. C. WHITMORE. n-butyl bromide reacts with dilute sodium amalgam giving a poor yield of mercury dibutyl. Mercuric chloride reacts with butyl magnesium bromide in excess, forming butyl mercuric bromide, calomel, and a small amount of mercury dibutyl. Using an excess of mercuric chloride gives a good yield of butyl mercuric bromide. Heating this substance with an excess of butyl magnesium bromide gives a fair yield of mercury dibutyl, a liquid boiling at 215°. The halide reacts with silver oxide and water giving a water solution of butyl mercuric hydroxide, a strong base. Treatment of this water solution with solutions of sodium halides gives precipitates of the butyl mercuric halides. The mercury butyl compounds are extremely toxic.

Mercury derivatives of meta-nitrobenzoic acid. Preliminary paper: F. C. WHITMORE and V. E. MEHARG. Nitrobenzoic acid fused with mercuric acetate gives a mixture of organic mercury compounds which are stable to sulfides. One of the compounds is 4-Hydroxymercuri-3-nitrobenzoic acid. The position of the mercury is proved by conversion into 4-bromo-3-nitrobenzoic acid. The mercury compounds are soluble in sodium hydroxide and in sodium carbonate. The solution in alkali causes a partial breaking of the C-Hg linkage. The mercury compounds and their reactions are being studied further.

The quantitative determination of paraformaldehyde: PARRY BORGSTROM and W. GRENVILLE HORSCH. Paraformaldehyde was analyzed by four methods and gave by (1) "neutral sulfite" 96.7 per cent., (2) iodimetric 96.7 per cent., (3) oxidation by dichromate, titration of excess 96.3 per cent., (4) oxidation by permanganate (or dichromate) with absorpton of CO_2 96.9 per cent. formaldehyde. For the "neutral sulfite" use 0.5 *N* to 1.0 *N* sulfuric or hydrochloric acid, rosolic acid as indicator and 4 *N* freshly prepared sodium sulfite. In the "iodimetric" method, the base is first added to the paraformaldehyde, then the iodine solution (0.2 *N*) within one minute time interval. Increasing this time or changing the order in which the reagents are added lowers the apparent formaldehyde content considerably. The remainder of the analysis is as usual. With other methods, ordinary precautions should be observed.

Rearrangements of some new hydroxamic acids related to heterocyclic acids and to diphenyl and triphenylacetic acid: Lauder W. Jones and Charles D. Hurd. Hydroxamic acids of thiophene and furane alpha carboxylic acids yield silver salts of their acyl derivatives which are less readily rearranged than the corresponding derivatives of benzhydroxamic acid. Diphenyl- and triphenylacethydroxamic acids were also examined, and it was found that an increase in the number of phenyl groups occasions greater readiness to rearrange. Diphenylacethydroxamic acid is formed by the action of diphenylketene upon hydroxylamine—a new type of reaction. Triphenylacethydroxamic acid is not produced by the interaction of ethyl triphenylacetate with hydroxylamine, but is quantitatively formed from the acid chloride.

The hydroxamic acid of cyclopropane carboxylic acid and its derivatives: Lauder W. Jones and Alfred W. Scott. The monohydroxamic acid



was prepared in order to determine what effect the trimethylene ring would have upon the Beckmann rearrangement of this compound and some of its derivatives. The hydroxamic acid is a colorless solid which melts at 123°. The benzoyl ester (*A*) C_3H_5—CO.NHO.CO.C_6H_5 (m. p. 149°) and the acetyl ester (*B*) C_3H_5—CO.NHO.CO.CH_3 (m. p. 106°), as well as the potassium, sodium and silver salts of these esters were studied. When the salts of (*A*) were heated gently, they decomposed to give cyclopropane isocyanate and the corresponding benzoates. Their relative stabilities increased in the order given above. When the salts of the alkali metals were heated with water, they showed a pronounced tendency to hydrolyze, which made it difficult to control the reaction so that the usual product of rearrangement, a sym-disubstituted urea, could be obtained. Unexpected stability was encountered in the case of the potassium salt of the acetyl ester (*B*) which is but little decomposed at 190°. The silver salt of this ester, unlike silver salts, was readily soluble in a mixture of alcohol and water, or in water alone.

The preparation of phenyl acetylene: John C. Hessler. Nef's method of preparing phenyl acetylene was to heat ω-bromstyrene in a sealed tube with alcoholic potassimu hydroxide. He used only a small quantity of alcohol in order to minimize the yield of the by-product, phenyl-vinyl ethyl ether. The writer's method is to allow the bromstyrene to flow, drop by drop, upon molten caustic potash contained in a flask heated in an oil bath at 200 to 220°. The phenyl acetylene distills over as it is formed, carrying with it only traces of unchanged bromstyrene. Yield: 80 per cent. of the theory of purified product.

On a quantitative study of the Grignard reagent: H. Gilman, P. D. Wilkinson and W. P. Fishel. In connection with some work on the addition of the Grignard reagent to ethylenic hydrocarbons, a method for the quantitative estimation of this reagent was found desirable. For this purpose a number of methods are being investigated, among them, (1) titration with iodine, (2) an indirect analysis involving the determination of the magnesium and alkyl halide actually used, and (3) an extension of the Zerewitinoff method involving the measurement of hydrocarbons given off when the Grignard reagent is treated with a compound containing "active" hydrogen. The first of these methods, that of titration with iodine, has been found unsuitable. In this connection the optimum conditions for the formation of the Grignard reagent are being studied.

A simple type of glass pressure bottle: R. R. Read. The apparatus consists of a simple adaptation of the common soda siphon to the purpose of a glass pressure flask.

An indirect method of mercurization of organic compounds and a method of carbon linking: Morris S. Kharasch. The method consists of heating the mercury salts of carboxylic acids, which lose carbon dioxide readily, the mercury then taking the position originally occupied by the carboxyl groups. Also, since the mercury can be readily replaced by a halogen, the method enables one to substitute a carboxyl group by a halogen. It was also found that the mercury compounds thus formed, especially those of the aliphatic series, can be made to split off mercury, thus linking the two carbon atoms. In the aromatic series, in the case of carboxylic acids which do not lose carbon dioxide readily, the mercury usually orients ortho to the carboxyl group. However, if a negative group is present in the molecule, the mercury orients itself ortho to that group, irrespective of the position of the negative group. In this respect, a number of substituted benzoic acids have been investigated.

Charles L. Parsons,
Secretary